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Date. 1903.	Exp. m	Date. 1903.	Exp. m
February 12	5	February 26	2, 2
17	7, 7	March 3	2, 2, 2
18	5, 5	6	2, 2
23	3, 4	11	1½, 1½
25	2, 3		

On the Desirableness of a Re-investigation of the Problems growing out of the Mean Motion of the Moon. By Simon Newcomb.

Should the question be asked, What is to-day the most important unsolved problem growing out of the celestial motions? a survey of the field could, it seems to me, lead to but one reply. It is that of the discrepancies between the observed mean motion of the Moon and the most exhaustive investigations of the theory of that motion. Twenty-five years have now passed since the publication of my "Researches on the Motion of the Moon,"* and during that time little of a comprehensive and definitive character has been added to the solution of the problem. A general review of it may therefore be appropriate at the present time. Such a review I now attempt.

I.

Beginning in a general way, we call to mind that, when slowly varying discrepancies are found between the predicted mean longitude of the Moon and that given by observation, two explanations are possible. One is that there is an actual deviation of the Moon from the theory on which the prediction is based; the other that the deviation is only an apparent one, due to a slow variation of the Earth's axial rotation, producing a corresponding change in our measure of time. Such being the case, the logical method of proceeding is to make an exhaustive investigation of the inequalities of long period in the mean motion of the Moon which may be produced by every known cause, to introduce any corrections which may be required to the theory, and then to attribute such deviations as remain outstanding to changes in the Earth's rotation. This is what I attempted in the work which I have quoted. The result was that the reconciliation of theory and observations in this way indicated changes in the rotation amounting in their accumulated effect to nearly half a minute of time in one direction or the other in the course of the last 250 years.

Such a result should, before being accepted as final, be tested

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* *Washington Observations for* 1875, Appendix II.

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by some independent method. Two conditions are available: the one based on the observed motion of *Mercury*, as determined by observations of its transits across the disc of the Sun; the other on the motion of *Jupiter's* first satellite, as determined from eclipses. The first of these conditions is the more precise, and is the only one which any serious attempt has been made to apply. In my discussion of observed transits of *Mercury* up to 1881* I found that seeming variations of the Earth's rotation were indicated, in which there was a fair agreement in algebraic sign with those inferred from the motion of the Moon, but of a magnitude which, in the general average, was little more than one-third as great. Again, in a paper presented to the French Academy of Sciences in 1896† I presented the results of a similar comparison brought up to the transit of 1894, based on the comparison of my new tables of *Mercury* and of the Sun. Only the November transits were used in this discussion, the May ones being of scarcely any weight. The results of this discussion and their comparison with those inferred from the motion of the Moon are summed up in the following table, where Δt is the amount by which the actual Earth must be supposed in advance of a uniformly rotating Earth. The dates 1690, 1741, and 1864 are the means for two transits; the others are those of the individual transits. In the case of the Moon the results are interpolated from the table found on p. 266 of "Researches."

	Δt.		
Date.	Mercury. s s	Moon. s	Diff. s
1690	+15 ± 10	+30	+15
1723	+ 8 ± 5	+17	+ 9
1741	+ 2 ± 4	+ 5	+ 3
1769	+ 6 ± 5	−12	−18
1789	− 8 ± 3	−18	−10
1802	− 4 ± 3	−16	−12
1822	+ 4 ± 5	− 9	−13
1848	− 7 ± 3	0	+ 7
1864	− 5 ± 2	+ 5	+10
1881	+ 4 ± 2	+16	+12
1894	+ 2 ± 2	+17	+15

It will be seen that in the case of *Mercury* the corrections to the times as indicated by observations are greater than their probable errors in eight out of the twelve cases, and in three cases are more than twice the probable error. Although they give colour to the hypothesis of variability in the measure of time, both by their magnitude and their constancy of sign from

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* *Astronomical Papers of the American Ephemeris*, vol. i. p. vi.

† *Comptes Rendus*, 1896, tom. I.

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1690 to 1769, they are not large enough to prove it. But in any case they are too small to be compatible with the large deviations inferred from the motion of the Moon. We may interpret the column of differences as the outstanding residual errors in the observed times of transits of *Mercury* in case we accept the results from the Moon as real.

The evidence seems almost conclusive that the very improbable deviations in the Earth's rotation inferred from the observation of the Moon are unreal, and that the motion of our satellite is really affected by causes which have, up to the present time, eluded investigation.

II.

The next questions are, What has been done in the way of investigating the possibility of such inequalities on the theoretical side, and by what means may we hope to reach a definitive conclusion on the subject? I need not go over in detail the researches of La Place and Poisson, and need only briefly allude to the work of Hansen, these being sufficiently set forth in the works already quoted. It will suffice to say that Hansen announced two inequalities of long period, due to the action of *Venus*, which he introduces into his tables of the Moon, and which, according to him, completely reconciled theory with observation. But within twenty years of the publication of his tables observation showed a rapidly increasing deviation, and a discussion of observations of occultations before 1750 showed discrepancies of more than 30″ in the latter part of the seventeenth century. It also turned out that one of these inequalities had no foundation in theory, and that Hansen's coefficient was so assigned as to represent observations from 1750 to 1850. This inequality must therefore be thrown out altogether. Four completely independent determinations of the other inequality have been made. Using the notation

g, the mean anomaly of the Moon;
E, the mean longitude of the Earth;
V, the mean longitude of *Venus*;
$A = 18\,V - 16\,E - g$;

these results are the following. All are reduced to the mass $1 \div 408{,}000$ of *Venus*.

Delaunay *	...	...	$16''{\cdot}34 \sin (A + 29^{\circ}{\cdot}5)$
Hansen † ...	...	...	$15{\cdot}34 \sin (A + 30{\cdot}2)$
Newcomb ‡	...	...	$14{\cdot}80 \sin (A + 30{\cdot}5)$
Radau § ...	...	...	$14{\cdot}14 \sin (A + 30{\cdot}0)$
Delaunay (cor.) ...		...	$14{\cdot}12 \sin (A + 29{\cdot}5)$

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* *Conn. des Temps*, 1862, addition. † *Tables de la Lune.*

‡ *Astronomical Papers of the American Ephemeris*, vol. v.

§ *Annales de l'Observatoire de Paris: Mémoires*, tom. xxi.

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Delaunay's result was corrected by Tisserand, and later by Radau, who concluded that it should be diminished by $2''\cdot2$ in consequence of terms omitted in the development. The last of the preceding expressions shows this corrected value.

It is impossible to say, until after a thorough re-investigation of the whole subject on an independent basis, that any of these results should be accepted to the entire exclusion of all the others. A strong presumption in favour of the Delaunay-Radau value is weakened only by the fact that those investigators used substantially the same method of development. But the close agreement of all four, taken in connection with the fact that the methods by which the computations are made are different from the beginning, seems to leave no room for doubt as to the general accuracy of the result, provided that we leave out of consideration quantities of the second order arising from the mutual perturbations of *Venus* and the Earth.*

During the period in question two attempts have been made to discover, by an exhaustive investigation, whether any undiscovered inequalities, due to the action of any planet, can possibly subsist. One of these was made by the writer, the next by Radau. In the first no attempt was made to determine the actual numerical value of the terms investigated, but only to set an upper limit to their possible value. Radau's work is more exhaustive. He attempts to determine the actual numerical value of every inequality due to the action of the planets, but finds none of sufficient magnitude to explain or materially reduce the discrepancy.

The only known causes to which we can attribute changes in the Moon's mean motion are the action of the planets and the action arising from the deviation of the Earth from a spherical form. Both actions have, to all appearances, been exhaustively investigated. Where, then, are we to look for a solution of the enigma? The whole question must be re-investigated *ab initio* with a view to discovering whether any omission or error has been made, either in the theory or in the complex process of comparing it with observations. As a sort of forlorn hope, it seems advisable to determine whether by any possibility the mutual perturbations of the Earth and *Venus* can give rise to a sensible term in the Moon's mean longitude. Moreover, every

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* In this connection a curious coincidence may be pointed out. In my "Researches" I showed that if we simply changed the sign of the constant term in the preceding expression, so that the argument should be A − 30° instead of A + 30°, the agreement between theory and observation would be fairly good throughout the entire period of 250 years of observation. So striking a coincidence led a writer in the *Vierteljahrsschrift der Ast. Gesellschaft* to accept the conclusion that the change was real, and that there was an error of sign in the term in question as used by Hansen. But this conclusion seems quite out of the question; in fact, I investigated it thoroughly before the publication of my work. It would seem, therefore, that we can look upon the case only as a singular coincidence.

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investigator who takes up the theory may well make a thorough inquiry into the question of the possibility of some periodic terms having been overlooked in the discussions so far made.

III.

What has been said in Section I. rests on comparisons of the observed mean longitude of the Moon at widely distant epochs with theory, the results of which are summed up on pages 264–266 of the "Researches" already alluded to. This comparison, though it seems fairly certain so far as general facts are concerned, may still need not unimportant corrections, especially during the period 1750–1840. It is remarkable that the history of the Moon's mean motion is more accurately known during the seventy-five years before 1750 than it is during the period 1750–1840, when the comparison of Greenwich observations with Hansen's tables commences. The results which I have cited rest on the hypothesis that during this period Hansen's tables represent the observations. In view of the fact that the tables are based on these observations, and that the few scattered comparisons which Hansen has given do not show large systematic deviations, we would seem to be justified in provisionally accepting this agreement. At the same time, with so enigmatical a problem before us, everything must be the subject of critical inquiry; and I deem it quite possible that from time to time the tables may be found to deviate from observations about the beginning of the nineteenth century to an extent greater than has been supposed.

Feeling the importance of the comparison in question, the writer, some twenty years ago, commenced and carried nearly to an end a work which he was obliged to suspend without bringing it to a definitive conclusion. It was that of comparing Hansen's tables with observations of occultations of stars since 1750. Occultations of stars by the Moon were taken as the basis for the work, for the reason that the results are less subject to systematic error, especially the personal error arising from comparing the limb of the Moon with a star, than are meridian observations. But the results are still subject to a not inconsiderable accidental error arising from the inequalities of the lunar surface. It is therefore desirable to supplement them by the Greenwich Meridian Observations. These, up to 1830, were reduced by Airy, and the results published in his monumental work fifty-five years ago. In a subsequent work the reductions were carried forward to 1851. The results were compared with Damoiseau's tables, to which were applied a number of corrections.

It does not seem that any certain result can be derived from this comparison at the present time. After making all allowances for the possible errors of the corrected tables and the probable errors of observation, the outstanding differences are larger than

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could be anticipated. Some years ago I endeavoured to investigate the subject by applying to the tables used by Airy the principal corrections necessary to reduce them to those of Hansen. But these corrections were too small to materially diminish the discrepancies. The case is rendered more embarrassing by the fact that when we take the few comparisons given by Hansen,* and compare them individually with those made by Airy, using the same observations, we can trace only the slightest relationship between the two.

As an example of this, I have made the following comparison of the two results at certain dates when the Moon was near the one or the other solstice, so that the residuals in longitude and latitude used by Airy should nearly correspond to residuals in R.A. and Dec. which are given by Hansen. Both sets are given in the sense Theory *minus* Obs.

	Δ Long.		Δ Lat.	
	Airy.	Hansen.	Airy.	Hansen.
	″	″	″	″
1751 Oct. 24	+ 2·1	+1·4	− 3·1	− 1·5
25	− 0·3	+3·5	− 6·5	− 5·2
Nov. 7	+15·2	+1·6	− 8·3	− 7·0
1752 Feb. 23	+ 3·8	−7·5	+ 2·2	+ 2·5
25	+15·4	+0·2	+ 6·0	+ 3·9
Mar. 6	− 3·4	− 6·6	+ 5·0	+ 6·1
7	+ 0·2	−3·7	+ 5·3	+ 2·8
9	+ 7·2	+0·7	+10·3	+ 3·7
Oct. 12	− 7·9	−3·5	+12·7	+11·2
13	− 8·1	−2·7	− 5·2	− 6·0
25	+ 3·1	−0·2	− 2·5	− 7·7
27	+ 1·7	+5·3	− 3·6	− 4·4
1824 Mar. 8	+ 7·7	0·0	− 0·2	+ 1·1
9	+ 0·6	−2·9	+ 0·3	− 0·6
Aug. 31	+ 8·0	+4·7	+ 6·3	+ 0·8
Sept. 1	− 1·3	+0·5	+ 2·8	− 2·5
2	− 4·0	+0·7	+ 8·1	− 0·6
15	+ 4·7	+0·7	+ 0·6	+ 0·4

When studying this subject twenty years ago I satisfied myself that these discrepancies could not be materially diminished by applying to Airy's results the differences between Damoiseau's theory, as corrected by Airy, and that of Hansen.

The investigation of the subject is rendered more difficult by the fact that no details of Hansen's comparison exist. The

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* *Monthly Notices R.A.S.*, xv. pp. 2–7.

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differences between theory and observation are all he gives. We find no statement as to methods or data of reduction, or the actually observed or computed positions of the Moon. Some twenty years ago, during a visit to Leipzig, I made an unavailing attempt to find the material in question among Hansen's papers, which are there preserved. A more thorough search might, however, bring something to light.

Whatever might be the outcome of such an investigation, it is very clear that Airy's reductions need a thorough revision, and that the results should be compared directly with Hansen's tables, the latter being corrected for their principal known errors. The great improvement made during the last half-century in our knowledge of the positions of the fixed stars, and in the data and methods of reduction generally, forms a sufficient reason for the suggested revision.

IV.

The question of correcting Hansen's tables before deriving any results from them is an important one. They undoubtedly need a number of corrections. Those first in importance, if we could only decide upon them, would comprise terms of long period in the mean longitude. But the nature of these terms is one of the very matters to be investigated, and it is therefore useless to make purely theoretical corrections that do not represent observations.

Among the corrections of short period, the most important is that required by Hansen's parallactic inequality. This is about 2″ too large in his tables. The existence of such an error is difficult to account for if the inequality in question was determined by a direct comparison of the theory with observations. A curious fact in this connection is that if the value which Hansen uses as applicable to his disturbed mean anomaly had been used as an inequality in the true longitude, the result would have been nearly correct. The question whether Hansen took the one coefficient as equivalent to the other is not now of practical importance. What is of practical importance is to correct the tables for the error, which is more troublesome than any other in the comparison with observations. I did not do this in my former "Researches," because the amount of the correction was still uncertain; and it is easier to make a correction like this, once for all, through a long period than it is to proceed by successive patching up.

I believe that all the other corrections to the tables are, individually, only fractions of a second, and will, in their total effect, rarely, if ever, rise to 2″. This total will also be a rapidly oscillating quantity, as often positive as negative. It can therefore not affect our conclusions as to the terms of long period.

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V.

The importance of the questions suggested in the preceding discussion is emphasised by the growing need of new tables of the Moon. During the half-century which has elapsed since the completion of Hansen's tables great progress has been made in determining the relations and numerical values of the various elements on which such tables must be based. The great stumbling-block in the way of a perfectly satisfactory basis is the enigma of the variations of long period. The solution of this enigma requires two processes. One is the thorough investigation of the mathematical theory of the subject, the other the discussion and comparison of observations.

In considering the mathematical side of the work the first question is that of method. Thirty years ago I proposed that the problem should be attacked in the most rigorous way by regarding the twelve elements of the Sun and Moon as simultaneously variable in the general Lagrangian equations of motion. An attempt by this method is found in a memoir on the action of the planets on the Moon, of which the work was actually done in 1871–72, though the memoir was not published until 1894. I am now convinced that this method requires developments too complex to practically be carried through. The developments actually found in it can, however, to a large extent, be used in a modification which leads to what is, in effect, a continuation of Delaunay's method, and is practically identical with the method adopted by Hill and Radau. What is wanted reduces itself to a rigorous reconstruction of the fundamental equations of this method.

On the observational side the question may arise whether it is not better to await the completion of an improved theory of the Sun's action on the Moon—for example, that which Professor E. W. Brown is now bringing to a conclusion—before attempting a correction of the lunar elements by observations. So far as the terms of long period are concerned it is clear to me that this question must be answered in the negative. A mapping out of these deviations, as observed through a period of 250 years, may afford valuable suggestions as to where we must look for their cause. Moreover, the results that may be reached will be practically as good when Hansen's tables are used, with a few preliminary corrections, as if we had the most accurate tables possible.

The case is not so strong for going on to the immediate determination of the lunar elements by taking Hansen's tables as the basis of comparison. If printed tables, based on Brown's theory, were available, it would be better to use them. Yet the advantage thus gained would not be so great as to justify a very long delay in order to realise it. The theoretical errors of Hansen's tables, some easily corrected faults aside, are so small

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that they would be completely eliminated in the mean of so long a series of observations as is now available.

More serious is the fact that the eccentricity and inclination which we should derive by the suggested plan would be those which belong to Hansen's system of variables, and not to the new theory. But it is so simple a matter to reduce one of these sets of elements to the other that this does not constitute a strong objection.

The following seems to me the logical order of proceeding as the case now stands :

1. A new investigation of the possible inequalities due to the action of the planets.
2. The completion of my comparison of Hansen's tables with occultations, in order to determine the actual deviations of long period.
3. The construction of provisional tables based on Brown's theory.
4. The re-reduction of the Greenwich Meridian observations since 1750.
5. The comparison of the results of this re-reduction with the tables of Brown or of Hansen.

The works here suggested involve such an amount of labour that a system of international co-operation in their prosecution seems desirable. The question whether my estimate of their importance is correct, and, if so, whether such co-operation is necessary and practicable, is one which I should be glad to see discussed.

Errata in Annual Report, Monthly Notices, vol. lxiii. No. 4 (*Obituary of Col. Cooper*).

Page 197, line 14 from bottom, *for* 1872 *read* 1863.
„ line 4 from bottom, *for* 1876 *read* 1874.